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Seatbelt Use Following Stricter Drunk Driving Regulations

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Abstract: We present evidence from the Fatality Analysis Reporting System and Behavioral Risk Factor Surveillance System that shows increased seatbelt use following the enactment of stricter BAC thresholds in states where seatbelt laws are primarily enforced. This suggests that inebriated drivers may use their seatbelts more judiciously to avoid being identified as a drunk driver by law enforcement. The interactive effect of stricter drunk driving laws and primary seatbelt laws are also shown to be more effective than either law passed in isolation in terms of reducing traffic fatalities.

Keywords: seatbelts, drunk driving
JEL codes: K4, I18

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1. Introduction

The National Transportation Safety Board (NTSB) recently proposed that all states move to a 0.05 maximum legal blood alcohol content (BAC) threshold for drivers. The 0.05 threshold is a common limit across the globe, and the NTSB predicts such a move would save many lives.[1] This discussion will likely spark renewed interest in the impact of lower BAC thresholds. Laboratory experimentation suggests a significant compromise to competence for drivers above BAC levels of 0.08 (Moskowitz, Fiorentino 2000). The number of lives saved, however, will not simply arise from the reduced fatality risk between the 0.05 and 0.08 thresholds implied by these experiments. Reductions in fatalities also depend on how drivers react to the changing threshold by adjusting their behavior and the impact of concurrent policies. For this reason, it is important to understand these responses so policies can be coordinated most effectively.

In this paper, we explore seatbelt use following BAC threshold changes from 0.10 to 0.08, which is a potential behavioral response among drivers that would affect the number of lives saved. Specifically, we hypothesize that drivers adjust their seatbelt use in light of BAC threshold changes in cases where local seatbelt laws are primarily enforced. Primary enforcement means that law enforcement personnel can pull over a driver if he or a passenger is observed to be not wearing a seatbelt. The mechanism is as follows. Drunk drivers anticipate an increased likelihood of detection in such cases where they are pulled over for an alternative infraction, e.g. failure to wear a seatbelt. Therefore, they avoid such interaction with law enforcement by complying with seatbelts

[1] See http://www.ntsb.gov/news/2013/130514.html (last accessed October 10, 2013).

laws. Ample evidence already suggests that there is a notable response to primary enforcement of seatbelt laws compared with secondary enforcement.[2] The penalties associated with BAC laws are far stricter than seatbelt laws, however, and are likely to interact with primary seatbelt laws to both increase belt use and reduce the number of fatal accidents.

Our analysis empirically confirms the hypotheses outlined above using multiple tests that employ several datasets. First, we appeal to the crash-level microdata of the Fatality Analysis Reporting System (FARS), which includes detailed information on every fatal accident in the U.S. We show that the interactive effect of a primarily enforced seatbelt law with a stricter BAC threshold (0.08 vs. 0.10) increases the likelihood that a driver in any fatal crash is wearing a seatbelt at the time a fatal accident occurs. The effects are concentrated among crashes that involve drivers with BAC levels at 0.08 or higher. We see no increase in seatbelt use, however, among sober drivers. Again, inebriated drivers may be more concerned with being pulled over, thus choosing to buckle up to avoid interaction with law enforcement. This first analysis only observes drivers involved in a fatal accident, however, which is a highly selected subsample. Moreover, we expect to observe fewer fatal crashes in total if there is more seatbelt use, so we also appeal to different tests and data to confirm our finding.[3]

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[2] Dinh-Zarr (2001) reviews the early evidence.

[3] Selection bias likely understates any measured effect, as an increase in seatbelt usage should reduce fatalities, and bias us from observing these individuals. Note that not all drivers involved in the accidents died, but that the accident involved at least one fatality.

In our second set of tests, we appeal to survey data using the Behavioral Risk Factor Surveillance System (BRFSS). In these data, seatbelt use is measured over the previous 30 days, and we can test whether there is any difference among drivers and passengers observed in response to stricter BAC thresholds. The evidence again points to a strong response in terms of general seatbelt use following BAC threshold implementation in states with primary seatbelt enforcement. The effect is concentrated among binge drinkers.

In our final analysis, we again appeal to the FARS, this time to establish that the stricter thresholds and primary enforcement of seatbelt laws indeed interact to save lives. We show in states with primarily enforced bans, coupled with a reduction in BAC from 0.10 to 0.08, there is a reduction in fatal accidents. This effect is concentrated in the evening and nighttime hours when a much higher percentage of drivers are potentially inebriated.

The paper proceeds as follows. Section 2 provides background of legislation regarding seatbelt use and BAC thresholds, including a review of the literature on the relevant behavioral responses of individuals. Section 3 describes our analysis of seatbelt usage, with additional explanations provided for the two data sources used. Section 4 presents our analysis of the effects of the laws on accident outcome and discusses implications. Section 5 concludes.

2. Background

2.1 A brief review of seatbelt and BAC threshold legislation in the U.S.

The first meaningful legislation pertaining to seatbelts in the U.S. was passed in 1961 in Wisconsin. All cars were required to be equipped with front-seat restraints but no mandates on usage were specified. Federal legislation followed in 1968, but as with the Wisconsin legislation, no requirements on usage were established. The first law that mandated that seatbelts actually be used was passed in New York in 1984. Many states followed with similar legislation, and currently only New Hampshire does not require seatbelt usage for adults.[4]

Table 1 summarizes the current status of seatbelt legislation across all states. With the exception of New Hampshire, legislation is either enforced as a primary or secondary offense. Recall that primary offense cases are those in which officers can pull a person over if they or a passenger are suspected of not wearing a seatbelt. This is where we would expect the interactive effects with concurrent BAC legislation.[5] Although the fines tend not to be severe, being pulled over for a primary offence opens up the possibility for the investigation of other potential offenses, such as alcohol, drugs, and weapons.

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[4] See http://www.iihs.org/iihs/topics/laws/safetybeltuse?topicName=safety-belts (last accessed October 10, 2013) for a chronology of such state legislation.

[5] There are also modest differences in age groups covered and fines. Given the penalties of drunk driving are much more severe, we view these minor differences as not meaningful sources of variation in this paper.

Legislation against driving while intoxicated in the U.S. has been in place for nearly as long as cars have been on the road. However, specific definitions of intoxication and enforcement of drunk driving laws emerged in the 1960s and continued over the next several decades. Initial laws set BAC minimums at 0.15, but states soon began establishing 0.08 or 0.10 as the more appropriate threshold. Currently, all states have a 0.08 threshold in place. Although many states conformed and codified this legislation voluntarily, legislation passed at the Federal level in 2000 prompted the remainder of the states to switch by 2004 or lose highway funds. Table 1 provides a list of dates when states moved to the 0.08 level during the sample period.

2.2 Effects of seat belt laws and BAC threshold laws

This paper links two strands of literature on driver behavior. The first is whether seatbelt laws encourage use and ultimately save lives. The literature has reached a general consensus that it does. A comprehensive review and meta-analysis was conducted over a decade ago by Dinh-Zarr et al. (2001). With regard to fatal injuries, the median finding across studies was a 9% reduction, and the range was 2% - 18%. Interestingly, non-fatal injuries barely changed, with many studies showing an increase in injuries. The mixed result is perhaps due in part to injuries among survivors of crashes who would have died had they not been wearing their seatbelt. Alternatively, passage of seatbelt laws may have led to a Peltzman effect, where drivers are more reckless with the increased perceived safety of seatbelts (Peltzman 1975).

Dinh-Zarr et al. (2001) also reviewed a number of studies that looked at the improvement of primarily enforced seatbelt laws over secondarily enforced laws and found that the median incremental decrease in fatal accidents was 8%. The specific study within this group that is most relevant to ours is Lange and Voas (1998). Although they considered just two communities in California, Salinas and Oceanside, they were the first (and one of the few) to assess seatbelt use among drunk drivers specifically. Their method of inquiry was periodic roadside surveys of drivers in these two communities, all of which were conducted between 9 PM and 2 AM. They find that nighttime seatbelt use increased dramatically after California moved from secondarily enforced bans to primarily enforced bans. Specifically, the jump was from 73% to 96% for all drivers in their survey. They further broke down the results by BAC level of drivers. For drivers with BAC content over the legal limit of 0.10 (at the time), the change was a much more substantial increase from 53% to 92%. The authors offer no specific explanation for this dramatic difference among drunk drivers, and the 1990s saw a number of changes that might confound the interpretation of this differential effect as being attributable to any particular policy. Nevertheless, based on the findings presented here we suspect that this result is related to the concern among inebriated drivers of being detected by law enforcement.

There have been a few additional studies of seatbelt use that are important for our purposes. First, Houston and Richardson (2005) employ a panel data approach to states enacting seatbelt legislation and find that seatbelt use increased by 9 percentage points more in states with primary laws compared with states with secondary laws. The scope of this study, which explored laws from all states utilizing a fixed effects approach, was more rigorous than previous work in establishing the importance of primary enforcement on the behavioral response to seatbelt usage.

The most relevant study for our purposes is Carpenter and Stehr (2008), who offer comprehensive evidence of the effects of different types of seatbelt laws on use and the incidence of fatal accidents. They also provide evidence of whether heavy drinkers were more or less likely to respond to seatbelt laws. Their findings confirm the general consensus in the literature that primarily enforced seatbelt laws are more effective than secondarily enforced bans. They also suggest that drinkers are not as responsive to seatbelt laws as nondrinkers, which is not entirely consistent with what was found by Lange and Voas (1998). This may be attributable to the sample of young adults used by Carpenter and Stehr, or it could be a result of the case study sample employed in the Lange and Voas paper. Nevertheless, what is not explored by Carpenter and Stehr, or anyone else, is the interaction between BAC laws and seatbelt laws.

Finally, the importance of aggressive seatbelt enforcement was recently highlighted by Lee (2013), who shows that a "Click-It or Ticket" campaign has a notable influence on traffic safety, particularly at night. The efficacy of such campaigns in the evening hearkens back to the work of Lange and Voas, and is consistent with our hypothesis. Specifically, drivers who are more concerned about being pulled over for being inebriated (much more common in the evening) would be more likely to buckle up. This makes it arguably most effective to test for effects on fatalities of the seatbelt laws and lower BAC laws in the evening.

The effectiveness of laws dictating BAC thresholds is more uncertain than seatbelt laws. There is a consensus of literature assessing positive influences of such restrictions, but these employ small samples or pay inadequate attention to alternative confounding influences. Three studies stand above these criticisms, but offer conflicting conclusions. Dee (1999) used the time and space variation afforded by the imposition of various state BAC thresholds to show a significant reduction in fatalities of stemming from both the 0.10 and the 0.08 threshold. Eisenberg (2003) makes the salient observation that the differential between these two estimates is most relevant since it represents the marginal changes most often observed in recent legislative actions. He indicates the Dee estimates imply a mere 2% reduction in accidents stemming from a reduced BAC threshold from 0.10 to 0.08. Eisenberg's own estimates imply a slightly larger impact of BAC threshold. He does note that this is somewhat more evident when one considers the effect of legislation with a delay.

Another recent paper by Chang et al. (2012) estimates the effect of seatbelt laws and lower BAC requirements, finding evidence consistent with the former having a greater effect on fatalities. The interactive effects are not explored, however, nor is the difference between primarily and secondarily enforced seatbelt laws.

A final paper relevant to our inquiry was conducted by Carpenter and Harris (2005), who examine drinking behaviors following the move from 0.10 BAC thresholds to 0.08 thresholds. Although they find some decline in alcohol consumption following these laws, they see no change in the likelihood of binge-drinking or alcohol-involved driving. The latter effects present some difficulties in reconciling these behavioral responses with lower fatality rates. Carpenter and Harris (2005) suggest that the reduced alcohol consumption represents a deterrent effect of laws, resulting in lower fatalities. We suspect this is part of the story, but our evidence offers an additional explanation. Even the binge drinkers and alcohol-impaired drivers are less likely to be involved in fatal accidents because their seatbelt use protects them and reduces the chance that any given crash results in a fatality.

3. Seatbelt Use Analysis

3.1 Data and Methods

We utilize two data sources to assess the impact of seatbelt use and BAC threshold legislation. The first is the FARS, which has the benefit of being a census of non-self-reported data. It is also the most reliable means to assess the ultimate impact of traffic legislation, which is the number of lives saved. Our final results will verify the interactive effect of seatbelt laws and BAC thresholds on fatal accident totals, making this dataset of primary importance to our study. Given our focus on seatbelt laws, we only use FARS accident data that involve passenger vehicles (as defined by the NHTSA), which include cars, light trucks, and vans.[6]

The first step in our inquiry is to ask whether actual seatbelt use differs among those who are likely most responsive to BAC threshold legislation in terms of their seatbelt use. Specifically, we look at drivers who become affected by a lower BAC

[6] The vast majority of the non-passenger vehicle-accidents observations excluded are those involving motorcycles.

threshold in a state with primary seatbelt enforcement. Our data span every month from 1991-2010, which captures a substantial number of states switching from BAC restrictions of 0.10 to 0.08. We estimate equation (1) as a probit model, with *s* and *t* indicating the state and month in which an accident took place.

$$(1) \qquad SBU_{ist} = \alpha_s + \tau_t + T_\tau \times \alpha_s + X_{ist}\beta + \delta PBL_{st} + \gamma BA08_{st} + \Psi BAC08_{st} * PBL_{ist} + \varepsilon_{ist}.$$

In order to answer our question, we first estimate equation (1) separately for drivers with a BAC of 0.08 or greater and for those with a BAC of 0. If the hypothesis of increased seatbelt use among inebriated drivers in response to stricter thresholds and primary belt enforcement is correct, the estimate of Ψ should be zero in the sober sample and significantly greater than zero in the intoxicated sample.

To formalize this comparison, we will also estimate equation (2), which is similar to equation (1) except it fully interacts the policy variables with *DWI*, an indicator set equal to one if the driver's BAC is greater than or equal to 0.08 and set equal to zero if his BAC is 0 (drivers with BAC between 0 and 0.08 are excluded from this estimation). In this case, we estimate

$$(2) \qquad SBU_{ist} = \alpha_s + \tau_t + T_\tau \times \alpha_s + X_{ist}\beta + \delta PBL_{st} + \gamma BA08_{st} + \phi DWI_{st} + \Psi_1 BAC08_{ct} * PBL_{ist} + \Psi_2 BAC08_{st} * DWI_{ist} + \Psi_3 DWI_{ist} * PBL_{ist} + \Psi_4 BAC08_{st} * DWI_{ist} * PBL_{ist} + \varepsilon_{ist}.$$

Equations (1) and (2) are estimated using driver-level data, with the *i* subscript indicating a driver. The variable SBU is a dichotomous variable indicating that a driver in a fatal accident uses a seatbelt. The regressions include a series of state fixed effects and year-month fixed effects. The former capture common differences in seatbelt use across states that are fixed with respect to time, while the latter allows for a unique intercept for seatbelt use for every time period in the sample. PBL is a dummy variable indicating a state has a primarily enforced seatbelt law in effect at the time of the accident. The BA08 dummy indicates a BAC laws of 0.08 is in effect in the state, as opposed to a 0.10 law. The interaction between the two and its corresponding coefficient estimate Ψ is the effect of primary interest in equation (1), while the triple interaction between both policies and the driver intoxication dummy (*DWI*) and its corresponding coefficient (Ψ_4) is the primary measure of interest in equation (2).[7]

In the X vector we add a number of factors that might affect the likelihood a driver wears a seatbelt. These include the driver's age and gender, a dichotomous measure of "good" weather conditions,[8] whether it was daytime (6:00am – 5:59pm), and whether the vehicle was a passenger car (vs. light truck or van). Finally, we include a series of dummy variables to account for the speed limit where the accident occurred.[9] Overall, the

[7] BAC levels are not available for all fatal crashes in the FARS even though such measures are required by law. We remove those cases where BAC levels are not directly measured.

[8] NHTSA classifies the prevailing atmospheric conditions that existed at the time of the crash as recorded on the crash report form. For our purposes we define the "good" weather condition dummy equal to one if NHSTA reports the weather as "clear" and zero if the weather condition is classified otherwise, which includes rain, hail, snow, fog, etc.

[9] Summary statistics for the FARS data used in equation (1) is provided in Appendix Table 1.

inclusion of these factors will capture variation over time in driver or road characteristics that may impact the likelihood of drivers in a particular state wearing a seatbelt, such as age and gender composition, weather conditions, or propensity for highway driving, for example.

There is a noteworthy drawback to using data drawn from the FARS. These are data points from fatal accidents and are necessarily highly selected. While the influence of selection bias should be to understate any measured effect, this is still a limitation. Therefore, we also appeal to the BRFSS, which includes monthly data on seatbelt use for a representative sample of the entire U.S. population and a myriad of additional information, including frequency of drinking. The BRFSS has the disadvantage of not recording whether the affected seatbelt users were actually making simultaneous drinking, driving, and seatbelt decisions, but it nevertheless confirms the FARS finding in a broader sample.

The BRFSS estimation proceeds by estimating (1) and (2) in a similar fashion, with a few notable differences. Equations (1) and (2) are ordered probit equations because the SBU variable in the BRFSS is an ordered measure. Specifically, seatbelt usage is self-reported in response to the question "How often do you use seat belts when you drive or ride in a car? Would you say..." as one of the following frequencies: "Always", "Nearly always", "Sometimes", "Seldom", and "Never". Not only does it utilize more information, but the ordered probit model is perhaps more telling of the seatbelt user behavior if the driver is marginally affected by the change in BAC threshold. That is, he may only buckle up when he fears he is within the threshold.

Among binge drinkers, this may lead to the driver moving along an ordered scale of seatbelt use frequency.

Another difference is the triple interaction indicator (*DWI*) used in equation (2). The BRFSS asks about alcohol consumption in the 30 days prior to the interview. Hence, the *DWI* indicator is exchanged with a *BINGE* variable, which represents the BRFSS survey question that asks whether an individual had 5 or more drinks in any one sitting in the past 30 days. We suspect that this common definition of binge drinking would identify those most affected by the law.

Lastly, we are able to include a richer set of demographic controls in vector X for this analysis, as the BRFSS data provide significant detail on individual characteristics (i.e. employment status, education, income, etc.) Table A2 summarizes the BRFSS sample used in the analysis.

Within the timeframe of the FARS analysis (1990-2010) the BRFSS seatbelt use question was asked annually and then later biennially, so we draw on the 1990-1998, 2002, 2006, 2008, and 2010 BRFSS survey waves. 2,063,436 participants responded to the seatbelt survey question in these survey waves. After dropping observations with missing demographic, income, or employment data, the final sample consists of 1,709,344 (approximately 17% of the sample is dropped).

3.2 Seatbelt use estimates using the FARS

We begin with an estimate of two versions of equation (1), which is a probit model predicting seatbelt use for all drivers in a fatal accident, and report the results in

Table 2. In column (1), the sample is drivers with measured BAC levels that exceed 0.08. The estimate of Ψ is 0.129 and is statistically significant at a high level (p-value = 0.001), which suggests a strong interactive effect of BAC laws in the presence of primarily enforced seatbelt legislation.

We calculate marginal effects in the bottom panel following suggestions by Greene (2010) for presenting interactive probit effects.[10] These estimates show that the incremental effect of imposing a stricter BAC threshold in a jurisdiction with a primarily enforced seatbelt law is a 4.22 percentage point increase in the likelihood of a legally drunk driver wearing a seatbelt relative to locations without a primary enforced seatbelt law. Given that in the sample the average seatbelt use of drunk drivers involved in a fatal accident is only 28%, this is a substantial effect.[11]

We recognize that if the effect reported in column (1) is consistent with our hypothesis (and not the result of unobserved confounders), a similar positive effect of the interaction of the primary seatbelt law and BAC threshold measure should not be present when investigating non-intoxicated drivers. Hence, we re-estimate equation (1) for a sample of drivers with zero BAC content. Results, presented in column (2) of Table 2, indicate that there is no impact of the interactive effect of lower BAC thresholds and primary seatbelt laws (p-value = 0.978). The primary belt law itself provides the only

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[10] These effects were calculated using the "margins r.policy1, over(r.policy2)" command in Stata (v12). Note that these are not naïve marginal effects that would ignore the calculated interaction coefficients, but are the first derivative of the conditional mean with respect to the policy of interest (Ai and Norton 2003, Greene 2010).

[11] Results are robust to restricting the sample to accidents involving a single car only.

effect, which is consistent with expectations. We note that the combined effect of primary seatbelt laws $(\delta + \Psi)$ in the inebriated sample exceeds that in the sober sample, which furthers confirms our hypothesis that drunk drivers are avoiding interaction with police in the primary belt law states.

While the difference between these estimates is consistent with our hypothesis and suggests that underlining trends are not an issue, we formalize our results in column (3) by explicitly adding a triple interaction between our policy variables and our DWI indicator, as detailed in equation (2). As suggested from the outcomes in columns (1) and (2), the triple interaction demonstrates the similar outcome, suggesting approximately a 4 percentage point increase in seatbelt use when both a BAC 0.08 threshold and primary seatbelt law are in place.

Lastly, we point out that the estimates of the control variables in Table 2 are as expected. This is notable given this is a sample of drivers involved in fatal crashes and therefore suggests selection may not be of substantial influence. Older drivers and women are more likely to be wearing seatbelts. Drivers wear seatbelts less in good weather and at night. The latter is likely because of the greater number of risky drivers on the road at night. Finally, we find that seatbelt use is highest on higher speed roads (e.g. highways).

3.3 Seatbelt use estimates using the BRFSS

While the results presented in section 3.2 indicate increased seatbelt use by those drivers who fear being pulled over, there is an important caveat to the Table 2 estimates just described. Specifically, the FARS data come from accidents with a fatality. If seatbelts result in fewer deaths, than it might be the case that we are observing a select group of more severe accidents. As argued earlier, the direction of the bias likely understates changes in seatbelt use, but we cannot confirm this definitively. For this reason, we turn to the BRFSS survey data to confirm the general findings of the FARS, estimating ordered probit versions of equations (1) and (2).

Table 3 presents these models, where the dependent variable is the frequency scale dependent variable of seatbelt use described above. Only the primary policy effects and their interactions are presented in this table, but the coefficients for the full set of controls are reported in Appendix Table A3. In columns (1) and (2), we test the simple interactive result of a lower BAC threshold and a primarily enforced ban separately for respondents reporting a binge drinking event or no drinking events in the previous 30 days. Thus, these two columns are estimated with a similar goal to the first two columns of the FARS estimates in Table 2. However, we do not know each respondent's BAC.

Evident from the estimates of Ψ in columns (1), the interactive effect of a primarily enforced ban and a lower BAC on binge drinkers is large and positive, which is consistent with the FARS analysis. That said, the results are not statistically significant. Unlike the FARS, the BRFSS does not directly link seatbelt use and drinking activity, which may reduce precision. Nevertheless, when comparing these estimates to those of non-drinkers (presented in the second column of table 3), we see that the interaction coefficient among binge drinkers is 4 times larger than that for non-drinkers.

We next combine the samples of binge and non-drinkers and fully interact an indicator of binge drinking in the previous 30 days with the policy variables in order to formally test the difference between the two groups, as shown in column (3). The first variable, entered in column (3), is an indicator of whether one binge drank in the last 30 days. Not surprisingly, binge drinkers are less likely to wear a seatbelt. Also, the triple interaction of binge drinking with both primary belt enforcement and a lower BAC threshold is positive and statistically significant. Taken collectively, these outcomes are consistent with the FARS results, as they show increased seatbelt use among drivers who would most like to avoid interaction with law enforcement.

Cross-partial effects, which can be interpreted as the difference in the probability of seatbelt use when reducing the BAC threshold to 0.08 under primary enforcement compared with reducing the BAC threshold similarly when not under primary enforcement, were also calculated using the estimated model coefficients and are reported in Table 4. The cross-partial effects of the interactions, or the double differences when comparing across the two policies, show a consistent shift away from reporting partial or no seatbelt use to always seatbelt use. This effect is also stronger among binge drinkers, reflecting the model coefficients presented in Table 3.

4. Accident Analysis

4.1 Data and Methods

Using two very different data sources, we have established that seatbelt use increases when BAC thresholds are lowered in the presence of primarily enforced seatbelt laws. Effects are only observed for drinkers, as expected. This leads to an important test and extension of our investigation—that is, whether the interactive effect of these laws saves more lives than the legislation passed in isolation. For these estimates, we return to the FARS data and aggregate fatal passenger vehicle accidents at the state level. The following equation assesses the relevant impact on such accidents:

$$(3)\qquad \ln(acc)_{st} = \alpha_s + \tau_t + T_\tau \text{ x } \alpha_s + X_{st}\beta + \delta PBL_{st} + \gamma BA08_{st} + \Psi BAC08_{st}{*}PBL_{ict} + \varepsilon_{ict}.$$

The log total of passenger-vehicle accidents in each state-month cell is estimated as a function of the same set of variables used earlier in the FARS. Estimations are weighted by the number of accidents to limit the undue influence of low-frequency, high variance states.[12] All standard errors are clustered at the state level to allow for non-independence of observation from the same location over time (Bertrand et al., 2004).

The X vector in these specifications also includes controls for population, which varies annually rather than monthly. Since state fixed effects capture the general size of the state, population reflects a change in relative density in this context. We round out the X vector with beer tax and unemployment rate controls. The former has been shown to reduce drunk driving accidents.[13] The latter represents depressed economic activity, which is likely associated with decreased traffic and accidents (Cotti and Tefft 2011).

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[12] While a weighted least squares approach is our preferred method, we confirmed that the results are robust to other specifications, such as OLS, Poisson, and Negative Binomial models.

[13] Ruhm (1996) finds beer taxes to be effective in deterring drunk driving for at least a subset of the population, while Eisenberg (2003), however, finds limited evidence of the effect of beer taxes.

Equation (3) results measure the various effects of legislation along with the incremental effect of a lower BAC threshold enacted in the presence a primary seatbelt law. Given that we are interested in the total number of alcohol-related accidents in each state and year, we can no longer exclude accidents where the BAC levels are missing. Instead, we use NHTSA-imputed BAC for all drivers where BAC is missing. As seatbelt use is a factor used by the NHTSA in imputing BAC levels, comparable estimates that divide the sample using BAC level (as was done in Table 2) are not appropriate for this analysis. We instead divide the sample into nighttime accidents (6:00pm – 5:59am) and daytime accidents (6:00am – 5:59pm).[14] We would expect a change in nighttime accidents, as this is the time period when the proportion of drivers that are intoxicated is highest.[15] Moreover, police presence to deter drunk driving is more prevalent in the evening. We would therefore also expect drivers to react more to legislation at night.

4.2 Fatal accident estimates using the FARS

The estimates in Table 5 support that nighttime accidents are most affected by the interactive effects of the legislation. There is a nearly 7% reduction in fatal accidents in states with a lower BAC threshold and a primarily enforced seatbelt law compared with a

[14] This is the formal nighttime vs. daytime convention used by the NHSTA when classifying accidents in the FARS data. We further restrict the daytime accidents to only weekdays (Monday-Friday) to further isolate the hours of the week when drunk driving is least prevalent form this sample.

[15] This approach is consistent with much of the literature that studies drunk driving accidents (e.g. Ruhm 1996, Eisenberg 2003). Notably, the BAC measures in the FARS data (both imputed and not) suggest that the proportion of nighttime accidents that involve alcohol is approximately 50%, while during daytime on weekdays the number is approximately 6%, suggesting that our bifurcation of the data is consistent with expectations and suitable for this investigation.

state without both laws in place.[16,17] Given that the average number of fatal nighttime passenger vehicle accidents per month in a typical state is approximately 27.6 in the sample, a 7% reduction translates into roughly two fewer fatal accidents per state per month, or over twelve hundred annually across the country. In fact, these estimates also suggest that there is no discernible reduction in fatal nighttime accidents unless both a primarily enforced seatbelt law and lower BAC threshold are in place, rather than with each law in place on its own.

In column (2) of Table 5 we contrast our nighttime findings to analogous estimates from daytime accidents. While daytime accidents are by no means exclusively non-alcohol related, there is a significant contrast based time of day, so we should see a much smaller or non-existent impact of the interaction on fatal accidents if our hypothesis is correct. Results are consistent with this expectation, as estimates of the interaction on outcomes are now half as large and no longer statistically significant. A conservative reading is that the negative estimate of Ψ observed in column (2) demonstrates the presence of an underlying difference in unobservables between treatment and control states in general accident rates that is correlated with the interaction of both policies in question. Consequently, if we difference the estimates between nighttime and daytime accidents we would "net" a negative effect of around a 3-4% decline in accidents. Due to

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[16] Results are highly robust to using an OLS, Poisson, or Negative Binominal specifications.

[17] For robustness purposes, we re-estimate these results isolating alcohol-related accidents, as classified by the BAC measures in the data, including the imputed alcohol data files from NHTSA. While this is likely inappropriate given the nature of the imputation process and the focus of our study on seatbelt laws, results are not sensitive to this approach.

the imprecision of the estimates, results are not statistically different from one another, so we are unable to completely eliminate this concern.

Among our other determinants of fatal accidents, it is worth noting that only beer taxes have a measureable impact on nighttime accidents, again consistent with drunk driving being more prevalent in the nighttime hours. We detect no effect of the state-level unemployment rate on nighttime accidents but a small decrease in daytime accidents.

5. Conclusion

This paper is the first to assess whether the effectiveness of stricter BAC thresholds are affected by another concurrent policy, namely seatbelt law enforcement. We suspected that primarily enforced laws raise the probability that a drunk driver would be detected by law enforcement. For this reason, we should observe heightened seatbelt use among drunk drivers.

Our data suggest that, indeed, seatbelt use increases among potentially inebriated drivers when BAC thresholds are lowered in areas with primarily enforced seatbelt laws. This is true in a census of fatal accidents. Drivers with a high BAC in such accidents are more likely to be wearing their seatbelt than in cases where there is no primary enforcement. This relationship is not observed in a sample of crashes where no driver was inebriated. We also look at the BRFSS, which retrospectively asks drivers about their seatbelt use. Among binge drinkers, frequency of seatbelt use increases when lower BAC thresholds are put in place where seatbelt laws are primarily enforced.

The critical question is whether the interactive effect of these policies is saving lives compared with either passed in isolation. We find that the laws, if enacted together, reduce accidents by 7% compared to cases with a secondary enforcement and a higher BAC level. More importantly, the effect of both laws is also considerably greater than either law passed in isolation. This provides a direct policy prescription when enacting stricter BAC thresholds. These laws should also be passed in an environment with strict seatbelt provisions if the intention is to maximize lives saved. We note that the observed reduction in fatal accidents is likely among drivers and their passengers, rather than other non-inebriated drivers and pedestrians. This means that drunk driving externalities, which are typically limited to lives lost outside of the car of the inebriated driver (Levitt and Porter 2001) are not substantially reduced. Nevertheless, the reduction in fatalities still is economically meaningful if one considers the contribution to society that can be made by those lives saved.

The results of the paper suggest several additional lines of inquiry. The evidence is highly suggestive that drivers actively avoid interaction with law enforcement. In the case of seatbelt laws and BAC legislation, the net effect is positive in terms of lives saved. This need not be the case. If seatbelt use leads to some illegal behavior not being detected, perhaps in the case of drug possession or weapons infractions, the net effect may not be positive. We anticipate more findings of the interactive effect of seatbelt law enforcement with other policies. We also suggest that future work on traffic safety should more generally look at the interactive effects of policies.

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Table 1. Primary Seatbelt Law and BAC Transitions

State	Any Belt Law in Effect	Belt Law: Primary Enforcement	Date of Transition to BAC 0.08
AL	7/18/1991	yes; effective 12/09/99	10/1995
AK	9/12/1990	yes; effective 05/01/06	9/2001
AZ	1/1/1991	no	9/2001
AR	7/15/1991	yes, effective 06/30/09	8/2001
CA	1/1/1986	yes; effective 01/01/93	1/1990
CO	7/1/1987	no	7/2004
CT	1/1/1986	yes; effective 01/01/86	7/2002
DE	1/1/1992	yes; effective 06/30/03	7/2004
DC	12/12/1985	yes; effective 10/01/97	4/1999
FL	7/1/1986	yes; effective 6/30/09	1/1994
GA	9/1/1988	yes; effective 07/01/96	7/2001
HI	12/16/1985	yes; effective 12/16/85	7/1995
ID	7/1/1986	no	7/1997
IL	1/1/1988	yes; effective 07/03/03	7/1997
IN	7/1/1987	yes; effective 07/01/98	7/2001
IA	7/1/1986	yes; effective 07/01/86	7/2003
KS	7/1/1986	yes; effective 6/10/10	7/1993
KY	7/15/1994	yes; effective 07/20/06	10/2000
LA	7/1/1986	yes; effective 09/01/95	10/2003
ME	12/26/1995	yes; effective 09/20/07	8/1988
MD	7/1/1986	yes; effective 10/01/97	10/2001
MA	2/1/1994	no	7/2003
MI	7/1/1985	yes; effective 04/01/00	10/2003
MN	8/1/1986	yes; effective 06/09/09	8/2005
MS	7/1/1994	yes; effective 05/27/06	7/2002
MO	9/28/1985	no	10/2001
MT	10/1/1987	no	4/2003
NE	1/1/1993	no	9/2001
NV	7/1/1987	no	9/2003
NH	n/a	no law	1/1994
NJ	3/1/1985	yes; effective 05/01/00	1/2004
NM	1/1/1986	yes; effective 01/01/86	1/1994
NY	12/1/1984	yes; effective 12/01/84	7/2003
NC	10/1/1985	yes; effective 12/01/06	10/1993
ND	7/14/1994	no	9/2003
OH	5/6/1986	no	7/2003
OK	2/1/1987	yes; effective 11/01/97	7/2001
OR	12/7/1990	yes; effective 12/07/90	10/1983
PA	11/23/1987	no	10/2003
RI	6/18/1991	yes; effective 6/30/11	7/2000
SC	7/1/1989	yes; effective 12/09/05	8/2003
SD	1/1/1995	no	7/2002
TN	4/21/1986	yes; effective 07/01/04	7/2003
TX	9/1/1985	yes; effective 09/01/85	9/1999
UT	4/28/1986	no	8/1983
VT	1/1/1994	no	7/1991
VA	1/1/1988	no	7/1994
WA	6/11/1986	yes; effective 07/01/02	1/1999
WV	9/1/1993	yes; effective 07/1/2013	5/2004
WI	12/1/1987	yes; effective 06/30/09	10/2003
WY	6/8/1989	no	7/2002

Sources: Insurance Institute for Highway Safety, National Highway Traffic Safety Administration, State Departments of Transportation.

Table 2. BAC 0.08 and Primary Enforcement Law Effects on Seatbelt Use Estimates, FARS

VARIABLES	1 Drinkers (BAC=0.08+) Seatbelt Use	2 Non-Drinkers (BAC=0) Seatbelt Use	3 Drinkers vs. Non-Drinkers Interaction Seatbelt Use
BA08	-0.0515	0.0052	-0.0052
	(0.0415)	(0.0307)	(0.0320)
PBL	0.0347	0.134***	0.130***
	(0.0503)	(0.0363)	(0.0487)
BA08 and PBL(Ψ)	0.129***	0.0011	-0.0062
	(0.0387)	(0.0389)	(0.0443)
Drunk Driver			-0.755***
			(0.0347)
BA08 and Drunk Driver			-0.0217
			(0.0489)
PBL and Drunk Driver			-0.0607
			(0.0617)
BA08 and PBL and Drunk Driver(Ψ_4)			0.117*
			(0.0678)
Age	0.00174*	0.004***	0.0035***
	(0.0009)	(0.0005)	(0.0005)
Male	-0.192***	-0.224***	-0.214***
	(0.0142)	(0.0110)	(0.0108)
Good Weather	-0.0804***	-0.0868***	-0.0842***
	(0.0123)	(0.0106)	(0.0072)
Daytime	0.0567***	0.0198*	0.0320***
	(0.0155)	(0.0113)	(0.0105)
Vehicle Type: Car	0.224***	0.155***	0.178***
	(0.0116)	(0.0205)	(0.0167)
Speed Limit <30	-0.242***	-0.187***	-0.200***
	(0.0324)	(0.0550)	(0.0418)
Speed Limit 30-39	-0.136***	-0.0697	-0.0876**
	(0.0320)	(0.0468)	(0.0365)
Speed Limit 40-49	-0.101***	-0.0123	-0.0402
	(0.0386)	(0.0379)	(0.0315)
Speed Limit 50-59	-0.226***	-0.114***	-0.149***
	(0.0250)	(0.0312)	(0.0232)
Speed Limit 60+	@	@	@
Observations	110,217	184,018	294,235

Partial Effects	Drinkers (BAC 0.08+)	Non-Drinkers (BAC=0)	
BA08	0.0012	0.0021	0.0008
PBL	0.0334	0.0498	0.0449
BA08 and PBL (cross-partial)	0.0422	0.0002	n/a
BA08 and PBL and Drunk Driver	n/a	n/a	0.0399

Notes: Both regressions include state and period (year-month) fixed effects. Robust standard errors clustered by state of residence are in parentheses. @ indicates excluded category. Sample includes FARS data from 1991-2010. *** p<0.01, ** p<0.05, * p<0.1

Table 3. BAC 0.08 and Primary Enforcement Law Effects, BRFSS Seatbelt Use Ordered Probits

	(1)	(2)	(3)
	Any binge drinking	No drinking	Combined
BA08	-0.0264	-0.0115	-0.0214
	(0.0311)	(0.0365)	(0.0352)
PBL	0.1213**	0.1162**	0.1217***
	(0.0565)	(0.0454)	(0.0459)
BA08 and PBL (Ψ)	0.0713	0.0177	0.0176
	(0.0560)	(0.0503)	(0.0509)
BINGE30			-0.2034***
			(0.0113)
BA08 and BINGE30			0.0252
			(0.0175)
PBL and BINGE30			-0.0132
			(0.0190)
BA08 and PBL and BINGE30 (Ψ_4)			0.0470*
			(0.0247)
Observations	845,860	215,364	1,061,224

Notes: All results are model coefficients and hypothesis tests (not partial effects). The sample consists of BRFSS waves in which respondents reported seatbelt use (1990-1998, 2002, 2006, 2008, 2010). Each column represents a separate regression. All models include controls for a respondent's demographics, income, employment, and indicators for year-month and state of residence. Robust standard errors clustered by state of residence are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 4. Ordered Probit Partial Effects, BRFSS

	Ordered Seatbelt Use (Ordered Probit)				
Predicted Outcome:	Never	Seldom	Sometimes	Nearly Always	Always
BA08 and PBL cross-partial, binge drinkers	-0.0053	-0.0039	-0.0054	-0.0065	0.0211
BA08 and PBL cross-partial, non-drinkers	-0.0012	-0.0007	-0.0012	-0.0018	0.0049
BA08 and PBL and Binge drinker cross-partial	-0.0012	-0.0014	-0.0029	-0.0054	0.0109

Notes: Results are cross-partial effects corresponding to the double- and triple-interaction models presented for the main BRFSS regression results. Hypothesis tests are not conducted, as recommended by Greene (2010), due to difficulties in their interpretation.

Table 5. Impact on State-Level Fatal Accidents

	(1) Ln Accidents Nighttime (6PM – 6AM)	(2) Ln Accidents Daytime/Weekdays (6AM – 6PM)
BA08	0.0417*	0.0679***
	(0.0242)	(0.0245)
PBL	0.0229	-0.00394
	(0.0374)	(0.0440)
BA08 and PBL (Ψ)	-0.0740**	-0.0362
	(0.0342)	(0.0416)
State Population in (100,000)	0.0025*	0.0020
	(0.0013)	(0.0017)
State Beer Tax	-0.530**	-0.241
	(0.210)	(0.148)
State Unemployment Rate	-0.0008	-0.0102
	(0.0082)	(0.0071)
Observations	12,101	11,912

Notes: Both regressions include state and period (year-month) fixed effects. Robust standard errors clustered by state of residence are in parentheses. Sample includes data from 1991-2010. *** p<0.01, ** p<0.05, * p<0.1

Table A1. Mean & Proportion, Fatality Analysis Reporting System, 1991-2010

	Drivers BAC=0.08+	Drivers BAC=0.00
Seatbelt Used	0.273	0.588
Age	33.55	41.51
Male	0.839	0.657
Good Weather	0.892	0.847
Daytime	0.186	0.640
Vehicle Type: car	0.579	0.609
Speed Limit <30	0.046	0.034
Speed Limit 30-39	0.184	0.136
Speed Limit 40-49	0.155	0.152
Speed Limit 50-59	0.483	0.488
Speed Limit 60+	0.131	0.190
Observations:	110,217	184,018

Table A2. Summary Statistics, Behavioral Risk Factor Surveillance System

	Mean/Proportion	Min	Max
Uses Seatbelt Always	0.761	0	1
Uses Seatbelt Nearly Always	0.123	0	1
Uses Seatbelt Sometimes	0.057	0	1
Uses Seatbelt Seldom	0.028	0	1
Uses Seatbelt Never	0.031	0	1
Any drinks in last 30 days	0.505	0	1
Any binge drinking events in last 30 days	0.126	0	1
Age	50.615	18	99
Male	0.404	0	1
White	0.858	0	1
Black	0.083	0	1
Hispanic	0.055	0	1
High School Grad	0.308	0	1
Some College	0.272	0	1
College Grad	0.317	0	1
Married	0.562	0	1
Current Smoker	0.200	0	1
Income $10k to $15k	0.071	0	1
Income $15k to $20k	0.086	0	1
Income $20k to $25k	0.104	0	1
Income $25k to $35k	0.145	0	1
Income $35k to $50k	0.169	0	1
Income > $50k	0.351	0	1
Employed for wages	0.501	0	1
Self-employed	0.089	0	1
Out of work for > 1 year	0.020	0	1
Out work for < 1 year	0.025	0	1
Homemaker	0.074	0	1
Student	0.022	0	1
Retired	0.220	0	1
Unable to work	0.047	0	1

Notes: *N* = 1,709,344. Summary of observations without non-responses, as described in the text. The sample consists of BRFSS waves in which respondents reported seatbelt use (1990-1998, 2002, 2006, 2008, 2010).

Table A3. BAC 0.08 and Primary Enforcement Law Effects, BRFSS Seatbelt Use Ordered Probits (All Coefficients)

	(1)	(2)	(3)
	Any binge drinking	No drinking	Combined
BA08	-0.0264	-0.0115	-0.0214
	(0.0311)	(0.0365)	(0.0352)
PBL	0.1213**	0.1162**	0.1217***
	(0.0565)	(0.0454)	(0.0459)
BA08 and PBL	0.0713	0.0177	0.0176
	(0.0560)	(0.0503)	(0.0509)
BINGE30			-0.2034***
			(0.0113)
BA08 and BINGE30			0.0252
			(0.0175)
PBL and BINGE30			-0.0132
			(0.0190)
BA08 and PBL and BINGE30			0.0470*
			(0.0247)
Age	0.0015	0.0209***	0.0055***
	(0.0012)	(0.0015)	(0.0011)
Age Squared	0.0000	-0.0002***	-0.0000**
	(0.0000)	(0.0000)	(0.0000)
Male	-0.3424***	-0.3857***	-0.3540***
	(0.0064)	(0.0108)	(0.0065)
White	-0.0499***	-0.0442**	-0.0464***
	(0.0168)	(0.0225)	(0.0158)
Black	-0.0650***	-0.0353	-0.0559***
	(0.0187)	(0.0288)	(0.0179)
Hispanic	0.1327***	0.1232***	0.1310***
	(0.0166)	(0.0191)	(0.0148)
High School Grad	0.0971***	0.1184***	0.0981***
	(0.0078)	(0.0111)	(0.0076)
Some College	0.1872***	0.2809***	0.2035***
	(0.0117)	(0.0149)	(0.0114)
College Grad	0.3341***	0.4945***	0.3703***
	(0.0157)	(0.0205)	(0.0163)
Married	0.0761***	0.1380***	0.0939***
	(0.0060)	(0.0073)	(0.0060)
Current Smoker	-0.1729***	-0.1581***	-0.1705***

	(0.0104)	(0.0121)	(0.0097)
Income $10k to $15k	0.0562***	0.0086	0.0484***
	(0.0065)	(0.0169)	(0.0064)
Income $15k to $20k	0.0863***	0.0290*	0.0755***
	(0.0073)	(0.0161)	(0.0069)
Income $20k to $25k	0.1003***	0.0371**	0.0869***
	(0.0096)	(0.0147)	(0.0090)
Income $25k to $35k	0.1048***	0.0416***	0.0901***
	(0.0099)	(0.0156)	(0.0101)
Income $35k to $50k	0.1268***	0.0741***	0.1142***
	(0.0107)	(0.0134)	(0.0101)
Income > $50k	0.1733***	0.1143***	0.1627***
	(0.0125)	(0.0175)	(0.0125)
Self-employed	-0.2216***	-0.2813***	-0.2375***
	(0.0084)	(0.0131)	(0.0086)
Out of work for > 1 year	-0.0462***	-0.0861***	-0.0545***
	(0.0103)	(0.0173)	(0.0088)
Out work for < 1 year	-0.0664***	-0.0555***	-0.0633***
	(0.0110)	(0.0136)	(0.0095)
Homemaker	0.0499***	0.0716***	0.0480***
	(0.0083)	(0.0152)	(0.0076)
Student	0.0773***	0.1162***	0.0884***
	(0.0136)	(0.0203)	(0.0126)
Retired	0.0889***	0.0578***	0.0795***
	(0.0064)	(0.0148)	(0.0062)
Unable to work	0.0242*	-0.0433	0.0135
	(0.0137)	(0.0271)	(0.0139)
Observations	845,860	215,364	1,061,224

Notes: All results are model coefficients and hypothesis tests (not partial effects). The sample consists of BRFSS waves in which respondents reported seatbelt use (1990-1998, 2002, 2006, 2008, 2010). Each column represents a separate regression. All models include indicators for year-month and state of residence. Robust standard errors clustered by state of residence are in parentheses. *** p<0.01, ** p<0.05, * p<0.1